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A DISSERTATION
ON THE
ORIGIN OF SPRINGS.

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PHILOSOPHY has long been desirous of investigating the *Causes* of things; but has usually made slow progress, where the *modus operandi*, or the connecting link between cause and effect, could neither be *seen* nor *felt*. To explain the phenomena of Thunder and Lightning, she first created a Jupiter, and then forged his thunderbolts. For the cause of Tempests, she resorted to the influence of the Stars. And HERODOTUS very gravely tells us, that the return of the Sun from the south, after the winter solstice, is owing to the prevalence of a violent South Wind in Egypt.

The *Origin of Springs* is a subject of this *invisible* and *impalpable* nature; and, as might have been expected, has long agitated the Republic of Letters. ARISTOTLE informs us, that the air, which is inclosed in the vaults and caverns of the Earth, is condensed into water near the surface; and thence flows out in springs. In the present state of science, probably, no one will think, that this scheme needs a refutation. Many au-

thors, however, who have treated it with an unbecoming ridicule, would have ſpared their wit, had they known, that their own hypotheſes, deviſed more than two thouſand years afterwards, were deſtined to a ſimilar fate with that of the Grecian Philoſopher.

The Modern Theories may be reduced to three claſſes. They all find the reſources of Springs

In the Ocean ;

In an Abyſs, in the bowels of the Earth ; or

In Vapour.

Thoſe, who draw the water of Springs from the Ocean, have been puzzled to convey it to their orifices in the ſtate in which we actually find it. Springs are uſually freſh : the Ocean is ſalt. Moſt Springs are higher, and ſome many thouſand feet higher, than the level of the Ocean. To raiſe their ſupplies requires, therefore, a force ſufficient to counteract the force of Gravity.

Theſe difficulties, attending the firſt claſs of theories, have coſt its advocates no ſmall expence of labour and ingenuity. Des Cartes, to avoid them, kindled a fire in the bowels of the Earth, by which he converted the water of the Ocean into Vapour. This Vapour, he collected in hollow ſubterranean caverns, and theſe condenſed it into water. Some of his followers, diſpenſing with theſe caverns, raiſed their vapour thus formed through the interſtices of the ground, until it was condenſed by cold near the ſurface. Had Des Cartes and his diſciples adopted the rule, which afterwards directed the reſearches of Newton ;

Never to aſcribe a phenomenon to any cauſe, until the exiſtence of that cauſe is proved ; their ſubterranean fire would never have been kindled. Perhaps, alſo, they would have found the remote cauſe of ſprings in the heat of a very different fire ; kindled, not merely in the imagination,—and by the hand of One, all whoſe theories are only practical.

The exiſtence of this ſubterranean fire is a mere hypotheſis,—wholly unſupported by proof. So far as the

interior of the Earth has been explored, its temperature below 1000 feet has been found, in all latitudes, to be about 50° of Fahrenheit : a temperature, which will appear wholly inconſiſtent with the exiſtence of ſuch a fire, if we attend to the following facts.— The Rivers now running have probably flowed ever ſince the Deluge ; and the quantity of water, which they now diſcharge, has doubtleſs been their average ſupply. Vapour is found to loſe 1000° of heat, when condenſed to water : and water abſorbs the ſame quantity, when reconverted into vapour. According to the theory, the vapour muſt riſe, before it is condenſed, at leaſt as high as the Springs which it ſupplies. What an incalculable quantity of heat muſt then have been diſcharged near the ſurface of the Earth, in order to ſupply the rivers with water ſince they firſt began to flow. This heat muſt have been conſtantly accumulating near the ſurface, during ſo long a period. Why, then, is it no where diſcovered ?

Others have attempted to avoid theſe objections, by calling in the aid of *capillary attraction.* As water is known to aſcend in *glaſs* tubes of a very ſmall bore, it is concluded that it may alſo aſcend through ducts in the Earth of a ſimilar ſize. Hence the exiſtence of ſuch ducts, and the aſcent of water through them, are taken for granted. The Springs, which feed the Amazon, are ſeveral thouſand miles from the Ocean ; and many of them iſſue from two to four miles above its level. He, who can believe, that the interior of the Earth is thus furniſhed with an apparatus of natural capillary aqueducts running horizontally thouſands of miles, till they meet a mountain fit to harbour a ſpring, and then aſcending perpendicularly to an orifice ; has faith enough and to ſpare.

But if theſe ducts actually exiſted, they would not explain the phenomena of Springs.—To whatever cauſe the riſe of water in capillary tubes is owing ; whether to the attraction *of the whole interior ſurface* of the tube ; or to the attraction *of the ring of glaſs contiguous to the*

upper surface; Reason demonstrates, that such a tube cannot *raise* more water than it can *sustain*; since the nearer the fluid is, the stronger is the attractive force. Experiment confirms this conclusion; for a capillary tube, however short, cannot be made to run over.—The force of capillary attraction, also, will raise water but to a very moderate height. The proposition found in most Treatises on Hydrostatics—*That the height to which fluids rise in capillary tubes, is inversely as the diameter of their bores*—has led many to the erroneous conclusion, that if the bore is indefinitely small, the fluid will ascend to an indefinite height. But the fact is otherwise. Dr. Hooke, after many experiments with what he calls *cobweb tubes*, was unable to raise water in them more than 21 inches.—Should we, therefore, suppose such ducts to exist in the earth of the requisite length and position, capillary attraction would not raise water in any of them to a greater height than 21 inches; nor, even in those of a less length, would it cause the fluid to run over.

Others allege, that if a small heap of ashes or sand is put into a bason of water, the water will rise through it, above its own level, to the top of the heap. They compare the Dry Land to the heap of ashes; and the Ocean to the water of the bason; and insist that the water of Springs rises in the same manner through hills and mountains.—I am not informed, that any experiments have been tried to ascertain the height, to which water will thus ascend through ashes, or through sand. The cause of its ascent, however, is well known to be a mutual attraction subsisting between the particles of ashes or sand, and those of water. Whether this attraction be chemical or mechanical, it is to me self evident, that no attraction can *raise* more water than it can *sustain*.—If it is *chemical*, it will continue to raise the fluid until the heap is thoroughly *saturated*, and no longer. Should the other particles of the fluid attempt to supplant those which were first combined, these would effectually resist the attempt by the right of *oc-*

cupancy. If the attraction is merely *mechanical*, its force is well known to diminish, *as the square of the distance increases.* The heap, therefore, being supposed to have raised as much water as it can sustain ; it is obvious, that the particles of water actually raised will be in immediate contact with the ashes or sand ; while all the other particles will be at a greater distance from it. The water *raised* will thus be more strongly attracted, than the water *not raised.* Of course, the ashes or sand will continue to sustain the particles first raised : and will not be compelled, by a weaker force, to let them go, and thus make room for others. In other words, a feebler attraction can not overcome one that is more powerful.

But it is said, that if we conceive of angular pipes or ducts in the Earth, having the form, position, and properties, of a *syphon ;* the water of the Ocean may ascend and be discharged through them, by the same laws which regulate that instrument. We will admit, for the argument, that it is strictly philosophical to conceive of ducts or pipes in the Earth *hermetically tight*, and of a sufficient extent to answer the purpose. This hypothesis will, nevertheless, be attended with an insuperable difficulty. It is well known, that the discharging orifice of the syphon must be horizontally lower than the surface of the reservoir. In the case supposed the Ocean is the reservoir. Of course, no spring, that is not somewhat below the level of the Ocean, can be supplied by a natural syphon. With the few springs so situated, we will not embarrass our enquiries : satisfied, that that WISDOM, which operates by general laws ; and, by the simplest means, produces the most magnificent results ; when it had filled the " *Upper Springs*, could find no difficulty in supplying the " *nether.*"

The *Wet Rag*, like the Syphon, has been called in to relieve the perplexities of Philosophy. It is well known, that if such a rag is thrown partly over the side of a vessel of water, the water will drop from the exterior end of the rag, until the whole is thus drawn off. The

Wet Rag in this case is a sort of clumsy natural syphon. But, like the more regular instruments of art, it is faithfully true to its principles of action. Unless the surface of the fluid is above the horizontal level of the exterior end of the Rag, it is found, that not a drop of water will fall.

None of these various schemes, therefore, if they would explain the ascent of water to the orifices of springs, will account for its running over.

Nor will either of them furnish the reason, why the water of springs is fresh. The water, when it begins to ascend, is mere brine; and is supposed to lose its salt in the passage. If it were true, that brine would thus become fresh, it would follow, that the lowest stratum of earth, or that nearest the Ocean, would be immediately, and the superior strata gradually, saturated with salt; and both would of course decline receiving any more from the brine, as it passed them on its way upward to the fountains. The brine would thus freshen less and less in its passage; and in the end would continue wholly salt. The lower strata, also, would soon be saturated with solid salt; that either the tubes, or the interstices, or the syphons, would become completely clogged, and incapable of any farther transmission of brine.

But it is not true, that sea-water is made fresh, by filtering through dry sand or dry earth. Experiment has proved, again and again, that, if brine is filtered through dry earth any number of times, its saltness is not perceptibly diminished. The quantity of fluid is indeed lessened; but that, which remains at the end of the process, is found to retain its original proportion of salt; and that, which is retained by the sand, adheres to it in the shape of *brine*, and not of *solid salt*. These considerations have satisfied me, that we must look to some other source than the Ocean, for the water of Springs.

The second class of Theories comprises those, which attribute the Origin of Springs to a vast Abyss in the bowels of the Earth.

When Philosophy discovered this Abyss, her *motives* were praise-worthy, whatever we may think of her *Logick.* Infidelity had often attacked the Scriptural account of the Deluge, on the ground, that all the water on the globe was not sufficient to cover its surface to the depth represented by Moses. This objection, if the fact it asserts were true, would rest on the unfounded principle, that the CREATOR of all things is dependent on *the things that are made* for the accomplishment of his purposes. Some well meaning friends of the Penteatuch, alarmed for the credit of Moses, devised this Abyss as the receptacle, in which the waters of the Deluge were gathered, *that they might no more overflow the face of the Earth.* Several philosophers, who had been put to great difficulty to account for the Origin of Springs, finding, in their subterranean researches, so copious a reservoir prepared to their hands ; immediately seized upon it as the source, whence they were supplied with water.

The best account I have seen of this Abyss, and of the manner in which fountains are fed by it, is found in Catcott's *Treatise on the Deluge :* a work declared by Jones and Adams, two distinguished philosophers of Great Britain, one of the last, the other of the present century, to be " the most critical and satisfactory discourse extant on the origin of Springs and Rivers."

Mr. Catcott explains his own view of the internal structure of the Earth, as it has existed since the Deluge, by an engraving, representing *the plane of one of its Great Circles.* " At the centre we find," to use his own language, " a solid ball, or *Nucleus*, of terrestrial matter, formed from what the water of the Deluge, in its descent from the surface and passage through the strata of the Earth, tore off, and carried down with it into the Abyss, and reposited at the lowest place. Around this Nucleus is the great *Abyss* of water, with which all seas, lakes and rivers communicate. This Abyss contains so large a quantity of water, that only a small part of it was used at the Deluge. Lastly, we

find the *Crust* of the Earth surrounding the Abyss, and broken into innumerable apertures and fissures; the largest of which are the beds of Lakes, Seas and Oceans; the next less are the canals for the waters of Springs; and the least, the cracks, through which the vapours of the Abyss ascend into the Atmosphere." To render this account more intelligible the Author compares the Earth to an *Egg*: its *Crust* to the *Shell*; the *Abyss*, to the *White*; and the *Nucleus*, to the *Yolk*. He then enters into an elaborate argument to prove the existence of this Abyss of waters; an attempt, in which he is pronounced successful by Jones and Adams.

Three methods are pointed out by Mr. Catcott, in which the waters of the Abyss may be conveyed to the orifices of Fountains:

By the ascent of Vapour through the Cracks in the Crust;
By Upward Filtration; and
By the Pressure of the Ocean.

The Vapour, mentioned in the first method, is supposed to be occasioned by a *fire*, existing somewhere in the bowels of the Earth. To avoid repeating what has already been said respecting an internal fire; I will barely remark, that such a fire can not exist in the *Nucleus*; for there can be no regular supply of air to support it: nor in the *Abyss*; for, had it once been kindled there, the water must immediately have extinguished it: and that if supposed to exist in the *Crust*, it could have no effect on the waters beneath. Were we, however, to allow the existence of such a fire in the *Nucleus*, operating on the bottom of the Abyss, like our common culinary fires on the bottom of a kittle: still, the Vapour thus occasioned would be condensed, in its ascent through many miles of cold water, long before it reached the under surface of the Crust. The only alternative is to imagine the Abyss to be, and to have always been, an immense subterranean ocean of *hot water*.—In this way, if we suppose 1st. That there is such an abyss; 2dly. That it is a mere mass of hot

water; 3dly. That the Cruſt of the Earth has the requiſite number of cracks and fiſſures; and 4thly. That, near the ſurface, the vapour finds the proper caverns or refrigeratories to condenſe in: we ſhall have made but two more ſuppoſitions than the Indian, who put the Earth on an Elephant, and the Elephant on a Tortoiſe.

If Vapour is thus continually riſing from the Abyſs, why do we never ſee it making its way above the ſurface in a viſible ſhape? To this queſtion, Mr. Catcott replies, "As to the aſcent of Vapour through the fiſſures of the Earth, this is a fact, the reality of which any one may be convinced of, who will give himſelf the trouble *of looking into the inſide of the Earth.*" Of the Earth, moſt of us, probably, are merely ſuperficial obſervers. For myſelf, unfortunately, I never had an opportunity of examining its *inſide* except while exploring a cave about 70 feet deep. In that, and in moſt other caverns and mines, water is ſeen dropping from the roof, and trickling down the ſides, of thoſe ſubterranean receſſes; and the atmoſphere, thus continually moiſtened, has no chance of being dried by the heat of the Sun. Miners, however, and moſt other viſitors of caverns, guided by common ſenſe, have been led to attribute the moiſture to theſe cauſes, and not to ſubterranean exhalations.

The ſecond method deviſed for the aſcent of the waters of the Abyſs is Upward Filtration. I flatter myſelf, that it has been already evinced—that water cannot, thus, aſcend to a ſufficient height:—That, if it could, it would not run over; and that, ſuppoſing it would run over, yet, if originally ſalt, it could not be made freſh.

With regard to the quality of the water of the Abyſs, Mr. Catcott obſerves, that "we cannot have any abſolute proof that it is ſalt; and I could give ſeveral reaſons to ſhow that it may not be ſo; at leaſt, not equally ſalt with the ſea." What theſe reaſons are he does not inform us. Perhaps it is owing to this o-

mission, that several reasons of a contrary efficacy have satisfied me, that the Abyss, even if it were originally fresh, must now consist of salt water. The Deluge prevailed on the Earth *one hundred and fifty* days. The waters of the Abyss, according to Mr. C. were employed, as subsidiary to those of the Ocean, in drowning the *dry land;* and then were returned *to their appointed bed*. Great indeed must have been the care, taken during that long period, to prevent the brine of the Ocean from intermixing with the fresh water of the Abyss.

The Ocean, also, according to Mr C. communicates directly with the waters of the Abyss; or in other words, is a part or continuation of it. If this be the case, the waters of the Abyss must be at least as salt as those of the Ocean; for salt water is heavier than fresh, and will sink in it until there is produced an equilibrium of specific gravity.

If the Ocean is not a part of the Abyss, but rests upon a substratum of earth, which in its turn rests upon the surface of the Abyss; then, no reason can be assigned why a due proportion of the cracks and fissures in the crust should not be assigned to the bed of the Ocean. Only one fifth of the surface of the globe is dry land. The remaining four fifths are covered by the Sea. The Dry Land, according to the hypothesis, has as many such fissures as it has Springs. And singular indeed must have been the care necessary after the Deluge, in settling the Wreck of Elements, to distribute these fissures in such a manner, that so many should be found in the Land, and not one in the bottom of the Ocean.

If these cracks and fissures are impartially distributed over the Crust of the Earth; it is clear, that the waters beneath must be as salt as those above. Let us suppose that, during the Deluge, the waters of the Abyss, by a strange coyness, were preserved from contamination; and that, after their *briny ordeal*, they returned *pure* to their native bed. Still they could only have returned to meet dangers insurmountable. The brine of

the Ocean, being ſpecifically heavier, muſt immediately have begun to deſcend through ſome of the cracks, and the water of the Abyſs to aſcend through others. Nor could the proceſs have ceaſed, until it had produced an equilibrium of preſſure. If this rotation in the waters of our Great Globe is not yet over, it will account, in a manner which Halley never dreamed of, for whirlpools, edies, and waterſpouts.

The third mode contrived for the aſcent of the water of the Abyſs is the preſſure of the Ocean. This Preſſure is thus explained.—A crack or fiſſure paſſes downward, from the orifice of every ſpring, through the Cruſt, to the ſurface of the Abyſs, which is ſuppoſed to be freſh. If water is poured into one arm of a bended tube, and oil into the other; the level of the oil, as being the lighter fluid, will remain higher than that of the water. The whole cavity of the Ocean may be conſidered as one arm of ſuch a tube; a given crack or fiſſure, the other; and the Abyſs, the connection between them. The brine of the Ocean, preſſing upon the lighter fluid of the Abyſs, will force it up through the fiſſure, to a greater height than its own level, and thus will form a ſpring.

On this ſcheme, the Ocean muſt be *connected* with the Abyſs; for, if it *is not*, it cannot preſs upon it. If it *is* connected with it, then I think it has been proved, that the waters of the latter muſt be ſalt. If they are ſalt, the preſſure of the Ocean cannot raiſe them above its own level; for, if water alone is poured into a bended tube, one arm of which is a hundred or a thouſand times as large as the other, ſtill the ſurface of the fluid in both will have the ſame horizontal level.

But if this preſſure *would* raiſe the waters of the Abyſs to a ſufficient height; ſtill, it would not make them freſh. The cracks or fiſſures are mere tubes of a *palpable* diameter; and to freſhen brine, merely by paſſing it through a tube, is a harder problem than a diſcreet Chemiſt would attempt to ſolve.

The manner in which ſprings iſſue from the ground,

alſo, completely diſproves the theory. Springs make their way nearly to the tops of the Andes. If the preſſure of the Ocean is ſufficient to force the water of the Abyſs to ſuch a height; but little of its force can have been ſpent in ſuch ſprings as iſſue on plains. In theſe, therefore, the water, inſtead of merely gurgling upwards, as they actually do, ſhould riſe in *jets;* and contiue riſing, until the impulſe communicated by this preſſure was overcome by the reſiſtance of the air, and the Force of Gravity.

As to the length of the cracks or fiſſures connecting Springs with the Abyſs, it ſhould be remarked that the *Cruſt* or *Shell* of the Earth cannot, on any reaſonable ſuppoſition, be conſidered as leſs than ſeveral miles in thickneſs. In a Globe of 8000 miles diameter, with a centrifugal force at its equator ſo powerful as that of the Earth; it will be readily *felt* by every man, that unleſs the Cruſt were ſolid and ſubſtantial, there would be conſtant and imminent hazard of ſomething worſe than a mere *fiſſure*. Springs, alſo, are often two or three miles above the level of the Ocean. Each Spring muſt have its own fiſſure commencing at the ſurface of the Abyſs, and reaching through the Cruſt to the orifice. The man, who can believe that the SUPREME BEING could deviſe no eaſier and better mode of watering the Earth, muſt I think, be in great danger of *thinking him*, ſo far as his wiſdom is concerned, *altogether ſuch an one as himſelf.*

Leaving, then, our ſubterranean reſearches, we will examine the Theory, which diſcovers the Origin of Springs in Vapour.

Vapour, as here uſed, is a comprehenſive term, including all the water, that riſes from the ſurface of the Earth by evaporation; and all, that deſcends on it in the form of dew, fog, miſt, rain, ſnow, and hail.

This Theory may be thus explained. The Ocean is conſtantly loſing vaſt quantities of water by evaporation. Electrified bodies attract light ſubſtances floating near them. The Land is more highly electrified than the

Ocean. Moſt of the vapour thus raiſed is drawn from the ſea to the land. As mountains are more highly electrified than plains, they attract the great body of the vapour which retains the form of miſt; while of that, which deſcends in a more ſolid form, much more falls on the former than on the latter. The miſt on mountains condenſes, and is precipitated in water. This united with the water of rain and ſnow, penetrates the ſtrata of ſand and the lighter earth, till it is ſtopped in its courſe by more impervious ſubſtances; particularly ſtrata of clay. In theſe, it forms a baſon or reſervoir; from which, gradually working a paſſage, it iſſues out of the ſide of the hill, in the form of a Spring.

Dr. Halley was the inventor of this Theory. His attention was directed to the ſubject by the following fact. While buſied in making ſome celeſtial obſervations, on a hill in the iſland of St. Helena, he found, even when the ſky was perfectly clear, that the quantity of vapour collected on his lenſes, every few minutes, was ſo great, as wholly to impede his viſion.

The following well-known facts may be adduced in its ſupport.

Water, in the form of Vapour, is conſtantly riſing from the Sea, in very large quantities, and in a ſtate of freſhneſs.

Rains are far more frequent and copious on mountains, than on plains; and in mountainous countries, than thoſe that are level.

The earth on mountains is always moiſt; even during a drought.

Almoſt all ſprings iſſue out of the ſides of hills or mountains, or from lands adjoining them.

The loftieſt mountains yield the moſt numerous rivers; and the largeſt, alſo, where they are far enough from the ſea.

Brooks are uncommon in champagne countries; and in countries, which have a ſtiff clay on the ſurface.

Brooks and Rivers may univerſally be traced to hills or mountains.

Streams, wells, and fountains are fullest in the Spring. In the Autum, many of them are absolutely dried up.

These facts collectively prove, that the quantity of water deposited on mountains is very great; that they are well qualified to retain it for the supply of Springs; and that springs and rivers are all of them in part, and many of them wholly, supplied from this source.

Various objections are urged against this theory. The first is that water will not soak far enough into the ground. M. De La Hire, a French Philosopher, to prove this, tried the following experiment. He dug a hole in the lower terrace of the Observatory at Paris; and placed therein, eight feet under ground, a large leaden bason, inclined a little towards one of its angles. To this was soldered a pipe, 12 feet long; which, after a considerable descent, reached into an adjoining cellar. He then filled up the hole with a mixture of sand and loam. After having kept the bason, in this situation, 15 years, (the ground being constantly exposed to all the rains and snows that fell,) he could never observe that a single drop of water had passed through the pipe into the bason.

On this subject, I will make two remarks. 1st. Water will certainly filter *down*, as easily as it will filter *up*; The attraction between the sand and the water is in each case the same; and in filtering down, it has the very serious assistance of Gravity. If then, it will filter upwards many miles, it will certainly filter downwards eight feet. 2dly. Our own eyes teach us, that deep wells frequently fail in dry weather; and that wells which are 30 or 40 feet deep are often very obviously raised by the hard rain of a single night. Hence I am led to conclude, either that the pipe employed in the experiment became clogged; or that Providence knows how to arrange earth, for the passage of water through it, better than M. De La Hire.

It is likewise objected that springs often rise on plains, and sometimes on the summits of hills. Such occurrences are uncommon. Of the few Springs which I

have heard of as riſing on the tops of hills, almoſt all have been found, on examination, to iſſue many feet below the real apex. Were the fact otherwiſe, it would not be inconſiſtent with the Theory.—In ſtiff clayed grounds water may work its way a very conſiderable diſtance, before it finds an outlet. Springs, in wet lands, ſhould be expected often to iſſue ſeveral miles from the reſervoir. "And," in the language of Hutton, "if there happen to be a valley, between a mountain on whoſe top is a ſpring and the mountain which is to furniſh it with water; the Spring muſt be conſidered as water conducted from a reſervoir of a certain height, through a ſubterranean channel, to make a jet of an almoſt equal height."

It is alſo ſaid, that ſome ſprings are not at all affected by drought. This may be owing to the capacity of the reſervoir; and to the number of ducts which ſupply it, and to the ſmallneſs of the drain.

But the principal objection is the inſufficiency of vapour to ſupply the demands of ſprings and rivers.

Dr. Halley tried the following experiment, to determine the actual evaporation from the Mediterranean, ſo far as it is occaſioned by heat. He filled a baſon with brine, as ſalt as that of the Ocean; and heated it, over a pan of coals, to the temperature of the air in ſummer. By a careful examination he found, that the quantity loſt by evaporation was a tenth of an inch in 12 hours. He ſuppoſes the Mediterranean to be 40° long, and 4° broad; making a ſurface of 160 ſquare degrees. According to the experiment, therefore, it will loſe 5,280,000,000 tons of water in a day. The Mediterranean receives the waters of the following conſiderable rivers; the Ebro, the Rhone, the Tyber, the Po, the Danube, the Neiſter, the Neiper, the Don, and the Nile. Dr. Halley ſuppoſed, that, on an average, each of theſe yields ten times as much water as the Thames; whereby he allowed for ſmaller rivers which fall into the ſame ſea. From an ingenious menſuration, he concluded, that the Thames diſcharges daily 20,300,000 tons.

The nine rivers, therefore, discharge into the Mediterranean 1,827,000,000 tons of water in a day; which is but little more than a third of what is raised from its surface in the same time, by evaporation.

The above estimate of the water of the Thames is professedly overrated, and has since been found, by Mr. Dalton the Philosopher of Manchester, to be about one third two large. Reducing the nine rivers in this proportion, we shall find that their daily discharge is only 1,218,000,000 tons This is rather less than 1-4th of the quantity evaporated from that sea; and leaves 4,062,000,000 tons, or 3-4ths of the whole, to meet the evaporation from that immense region, by which these rivers and their branches are supplied.

From a series of observations respecting the annual supply of rain, made at 31 different stations in England, during different periods of from 1 to 21 years, Mr. Dalton finds the average quantity for the whole of England and Wales to be 31 inches. To this he adds 5 inches for dew; making a total of 36 inches or three feet. After a minute examination of the rivers of those two countries, he estimates their whole annual discharge to be nine times that of the Thames. Allowing this calculation to be correct, the rivers will exhaust only 13 inches of rain; leaving 23 inches to evaporate from the land: a residuum, which he finds, by a number of experiments, to be amply sufficient.

The quantity of rain, which falls in our own country, is believed to be considerably greater than that in any country of Europe. The exact amount however cannot, at present, be stated with precision; as few gentlemen have hitherto made the requisite observations. I can only observe, that I have seen various registers of rain kept at different places for considerable periods: and the average results of a still larger number. From these I am led to conclude, that the quantity of water, which annually falls in rain snow and hail, will average 45 inches. To this should be added 5 inches, at least, for dew; making the whole 50 inches. The United

States comprise a million of square miles. Thirty five cubic feet of rain water weigh a ton. A mass of water, covering 1,000,000 square miles to the depth of 4 feet 2 inches, contains 116,160,000,000,000 cubic feet; amounting to 3,318,860,000,000 tons.

By an examination of the Map of the United States it will appear, that, on its borders and within it, there are 26 great *primary* rivers, or rivers communicating directly with the Ocean These are the St. Lawrence, Penobscot, Kennebec, Ameriscoggin, Saco, Merrimac, Connecticut, Hudson, Delaware, and Susquehannah, in the North; and the Potowmac, Rappahanoc, York, James, Roanoke, Pamlico, Neuse, Cape Fear, Pedee, Santee, Savannah, Ogeechee, Altamaha, Apalachicola, Mobille, and Mississippi in the South. The St. Lawrence receives half of its water from the Canadas; and more than three fourths of the supplies of the Mississippi are furnished by Louisiana. Of this the proof is direct. The *Ohio*, the only very large tributary from the east, is smaller than the *Arkansas*; and the *Illinois*, the only remaining eastern branch worthy of notice, is much less than the *St. Francis*. On the West, however there are also the *Missouri*, which is larger than the Mississippi itself before the junction, added to all the branches from the east; and *Red River*, which is much larger than the Ohio.

Of the remaining *primary* rivers, the Ameriscoggin, Saco, Merrimac, Rappahannoc, York, Pamlico, Neuse, and Cape Fear, are about as large as the Thames; and the Penobscot, Kennebec, Ogeechee, and Altamaha, are not much larger. Taking these facts into consideration, I am satisfied, that, if we admit each of the 26 primary rivers to yield ten times as much water as the Thames, we shall allow a sufficient overplus to supply all the primary streamlets. On this supposition the annual discharge of water from all the rivers of the Union will amount to 1,281,660,000,000 tons: leaving a residuum of 2,037,200,000,000 tons to supply the demands of evaporation from the land.

I am aware, that ſuch calculations, in their own nature, do not admit of that certainty which is demanded in demonſtration. Still they are ſufficiently accurate to leave the mind ſatisfied. I am alſo aware, that their want of certainty comes with an ill grace from the mouth of the objector. As the advocates of the theory prove, in the outſet, that many ſprings are wholly ſupplied in this manner; the caſe with reſpect to the others, is *prima facie* with them. As the objector takes the iſſue, the burden of proof reſts upon his ſhoulders. Until it is actually furniſhed I believe we ſhall all admit, that Springs owe their origin only to Vapour.

No. XX.

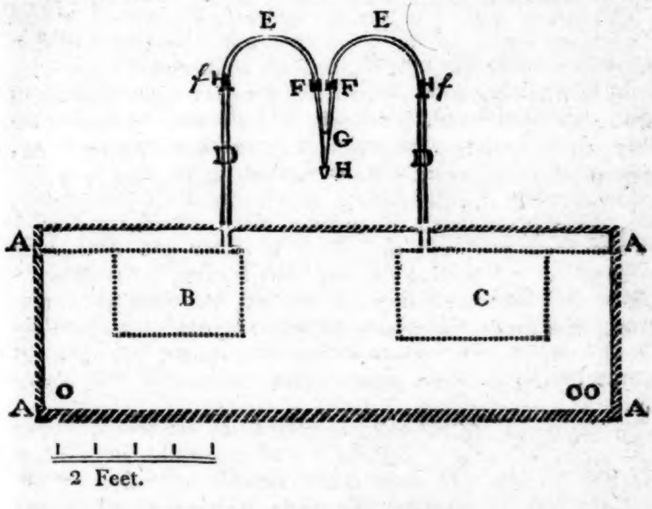
EXPERIMENTS

On the Fusion of various refractory Bodies, by the

COMPOUND BLOW-PIPE OF MR. HARE,

BY BENJAMIN SILLIMAN, PROF. CHEM. AND MIN. IN YALE-COLLEGE.

A SECTION OF THE **PNEUMATIC CISTERN** OF YALE COLLEGE WITH THE **COMPOUND BLOW PIPE** OF MR. HARE FOR BURNING HIDROGEN, MINGLED WITH OXIGEN GAS.



REFERENCES TO THE FIGURE.

AAAA.—The pneumatic cistern, filled with water; for a plate, and full description, see the **Boston Edition of Henry's** Chemistry.

B.—A Gas Reservoir, of the capacity of twelve gallons, filled with oxigen gas, either by the action of the hydrostatic bellows at O, or, by a recurved tube, passing from above, through the water, and hooked under **B**: parallel, and contiguous to B, on the other side of the cistern, is another gas reservoir, of the same capacity, which may be connected with B, or not, at pleasure.

C.—The same, in every respect, only C is filled with hidrogen, by hydrostatic bellows at OO, or by a recurved tube, as above.

D.—Copper Tubes, half an inch in diameter, furnished with stop cocks at *f*, and inserted into the gas reservoirs B. C.

E.—Recurved tubes of flexible metal, furnished with double screws at F, which connect them with a pair of brass blow pipes, cut off at G, and soldered to two strong cast silver tubes, which screw, *air tight*, into H, an inverted pyramidal piece of platinum, in which, two converging ducts as large as a pin are perforated, forming a continuation of the tubes, and uniting in a common passage, somewhat larger, just before their exit, at the common orifice below. The subject to be operated upon is sustained by charcoal, or forceps, and held by the hand, just below the orifice in the piece H.

The gasses at B. C. are under hydrostatic pressure, which is easily recruited, as the gasses run out, either by throwing common air with the bellows, into one of the spare reservoirs, or, by introducing more of either of the gasses into the appropriate reservoir, and, peculiarly of hidrogen, both, on account of the facility with which it is obtained, and because, twice as much of it, in bulk, is wanted, as of oxigen.

The rapidity of efflux of the gasses, and their due proportion, is easily regulated, by turning, more or less, the keys of the stop cocks at *f*, and the effects of either gas alone, may be observed, by shutting the stop cock leading to the other.

When the compound flame is desired, the hidrogen is first let out, and fired ; the blaze should be somewhat larger than that of a candle ; the oxigen is then let into the hidrogen till the effect is the greatest, which a little habit will soon ascertain.

The flame of the hidrogen is very much narrowed, by the introduction of oxigen, and there is no appearance of peculiar splendor or heat, till some body, capable of reflecting the light and heat, is placed in the focus, which is usually about one fourth of an inch below the orifice.

All the apparatus below F F is easily detached, by turning the double screws ;—the strong silver tubes are intended to prevent fusion of this part of the apparatus, and to admit of connexion with the platinum piece by *means of a screw cut on the silver tubes ;* this obviates the necessity of using a solder, which would be very liable to melt, and, the platinum piece is, for a similar reason, substituted for the silver cylinder originally used by Mr. Hare, as experience has shewn that these are liable to fusion.

No flux or addition of any kind was employed in the following experiments.

EXPERIMENTS

ON THE FUSION OF VARIOUS REFRACTORY BODIES BY THE COMPOUND BLOW-PIPE OF MR. HARE.

THE philoſophical world behold with pleaſure and aſtoniſhment, the effects produced on the fuſion and combuſtion of bodies, by a stream of oxygen gas, directed upon burning charcoal. The ſplendor of these experiments arreſted univerſal attention, and Lavoisier, with his gazometer, was enabled, in this manner, to produce a degree of heat, ſurpaſſing that of the moſt powerful furnaces, and even of the ſolar focus. Bodies which no degree of heat, previously applied, had been able to ſoften, now became fluid, and philoſophy appeared to have attained the limit of its power in exciting heat; indeed, it ſeemed to have advanced, very far, towards realizing the opinion, that ſolidity and fluidity are accidental attributes of bodies, dependant ſolely on the quantity of caloric which they contain, and that therefore, they may be ſuppoſed capable of exiſting in either of theſe conditions.

Still however, there were, *in fact*, many important exceptions. Of the primitive earths, Lavoisier had been enabled to fuſe only alumine—while the reſt remained refractory, and ſeemed fully entitled to the character of infuſibility, uſually attributed to this claſs of bodies: many native minerals and eſpecially thoſe which are moſt diſtinguiſhed for hardneſs, beauty, and ſimplicity of compoſition, maintained the ſame character, and ſome of them refuſed to melt even when heated with powerful fluxes.

The beautiful invention of Mr Robert Hare of Philadelphia, by which he ſucceeded in burning, with ſafety and convenience, the united ſtream of oxygen and hidrogen gases, greatly extended our dominion over refractory bodies, and preſented new and very intereſting reſults. Mr. Hare's memoir, originally communicated to the Chemical Society of Philadelphia, has been

some years, before the public, and has been republished and handsomely noticed, both in France and England. Still however, his results have not found their way into the Systematical books on Chemistry, (with the exception of Mr. Murrays system,) notwithstanding that some of the European Professors have availed themselves of Mr. Hare's invention, so far as to exhibit his most splendid and striking experiments to their classes.

The writer of this article, although fully disclaiming any share in Mr. Hare's invention, was early associated with him in his experiments; they excited in his mind a degree of interest, which led him to hope that they would be repeated and extended by others, but, as nothing of this kind has appeared in this country, perhaps the following experiments may not be altogether uninteresting, especially as they were performed with an apparatus, of a construction somewhat more simple than the original.

It will be necessary to recollect that Mr. Hare not only melted alumine, which Lavoisier had done before, but also *silex and barytes*, and, by subsequent experiments, he added *strontites*, to the list of fusible bodies: he was inclined to believe that he had volatilized gold and silver, a conclusion which was rendered highly propable by his having afterwards evidently volatilized platinum.

The experiments of Mr. Hare, as will appear below, have been repeated by the writer of this paper with success, and many other bodies among the most refractory in nature, have been melted. For the sake of shewing how far the experiments now to be recited have affected our knowledge of the dominion of heat, quotations, for comparison, will occasionally be made, from one of the latest and most respectable chemical authorities.

(Murray's System 2d Ed.)

Bodies submitted to the heat of the compound Blow-Pipe of Mr. Hare.

Primitive Earths.

Silex—being in a fine powder, it was blown away by the current of gas, but when moistened with water,

it became agglutinated by the heat, and was then perfectly fuſed into a colourleſs glaſs.

Alumine—perfectly fuſed, into a milk white enamel.

Barytes—fuſed immediately, with intumeſcence, owing to water, as obſerved by Lavoiſier; it then became ſolid and dry, but ſoon melted again into a perfect globule, a greyiſh white enamel.

Strontites—the ſame.

Glucine—perfectly fuſed into a white enamel.

Zircon—the ſame.

Lime—in ſmall pieces, it was immediately blown off from the charcoal; to prevent this, as well as to obviate the ſuſpicion, that any foreign matter had contributed to its fuſion, the following expedient was reſorted to. A piece of lime, from the Carrara marble, was ſtrongly ignited, in a covered platinum crucible; one angle of it was then ſhaped into a ſmall cylinder, about one fourth of an inch high, and ſomewhat thicker than a great pin: the cylinder remained in connection with the piece of lime: this was held by a pair of forceps, and thus the ſmall cylinder of lime was brought into contact with the heat, without danger of being blown away, and without a poſſibility of contamination; there was this farther advantage, (as the experiment was delicate and the determination of the reſult might be difficult,) that, as the cylinder was held in a perpendicular poſition, if the lime did really melt, the column muſt ſink and become, at leaſt to a degree, blended with the ſupporting maſs of lime. When the compound flame fell upon the lime, the ſplendor of the light was perfectly inſupportable, by the naked eye, and when viewed through deep coloured glaſſes (as indeed all theſe experiments ought to be,) the lime was ſeen to become rounded at the angles, and gradually to ſink, 'till, in the courſe of a few ſeconds, only a ſmall globular protuberance remained, and the maſs of ſupporting lime was alſo ſuperficially fuſed at the baſe of the column, through a ſpace of half an inch in diam-

eter. The protuberance, as well as the contiguous portion of lime, was converted into a perfectly white and glistening enamel; a magnifying glaſs diſcovered a few minute pores, but not the ſlighteſt earthy appearance. This experiment was repeated ſeveral times, and with uniform ſucceſs; may not lime therefore be added to the liſt of fuſible bodies?

Magnesia.—The ſame circumſtances that rendered the operating upon lime difficult, exiſted, in a ſtill greater degree, with reſpect to magneſia; its lightneſs and pulverulent form rendered it impoſſible to confine it for a moment upon the charcoal, and as it has very little coheſion, it could not be ſhaped by the knife as the lime had been. After being calcined, at full ignition, in a covered platinum crucible, it was kneaded with water, 'till it became of the conſiſtence of dough. It was then ſhaped into a rude cone as acute as might be, but ſtill very blunt; the cone was three fourths of an inch long, and was supported upon a coiled wire.

The magneſia, thus prepared, was expoſed to the compound flame: the eſcape of the water cauſed the vertex of the cone to fly off in repeated flakes, and the top of the fruſtrum, that thus remained gave nearly as powerful a reflection of light as the lime had done; from the bulk of the piece (it being now one fourth of an inch in diameter at the part where the flame was applied) no perceptible ſinking could be expected. After a few ſeconds, the piece being examined, with a magnifying glaſs, no roughneſſes or earthy particles could be perceived on the ſpot, but a number of glaſſy, ſmooth protuberances, whoſe ſurface was a perfectly white enamel; this experiment was repeated with the ſame ſucceſs. May not magneſia, then, be alſo added to the table of fuſible bodies?

Yttria—was the only remaining primitive earth, but no ſpecimen of it could be obtained.

Perhaps then we ſhall be juſtified in ſaying, in future, that the primitive earths are fuſible bodies, although not fuſible in furnaces, in the ſolar focus, nor, (with the exception of alumine, and poſſibly, barytes,) even by a ſtream of oxygen gas directed upon burning charcoal.

Platinum—was not only melted but volatized with ſtrong ebullition.

Various Minerals.

Rock Crystal,—tranſparent and colourleſs. This mineral was inſtantly melted into a beautiful white glaſs. "It not only does not melt in the focus of the moſt powerful burning mirror, but, it remains without fuſion, at leaſt when in the ſtate of Rock Cryſtal, in the ſtill more intenſe heat, excited by a ſtream of oxigen gas directed on burning charcoal." (Murray II. 261.) "It is even imperfectly ſoftened by the intenſe heat, excited by a ſtream of oxigen gas, directed on the flame." (of the blow pipe lamp.) —(Ibid III. 513.)

Common Quartz—fuſed immediately into a vitreous globule.

Gun Flint–melted with equal rapidity; it firſt became white, and the fuſion was attended with ebullition and a ſeparation of numerous ſmall ignited globules which ſeemed to burn away as they rolled out of the current of flame; the product of this fuſion was a beautiful ſplendid enamel.—"It is infuſible before the blow pipe but loſes its colour."—(Ibid. 518.)

Chalcedony—melted rapidly, and gave a beautiful bluiſh white enamel reſembling opal. "It is infuſiblebefore the blow pipe."—(Ibid. 516.)

Oriental Carnelian—fuſed with ebullition, and produced a ſemitransparent white globule with a fine luſtre.

Red Jasper—from the Grampians, was ſlowly fuſed with a ſluggiſh efferveſence, it gave a greyiſh black ſlag, with white ſpots.

"It is infusible before the blow pipe, even when the flame is excited by a stream of oxygen gas." (Ibid. 519.)

Smoky Quartz—or smoky topaz melted into a colourless globule.

Beryl—melted instantly, into a perfect globule, and continued in a violent ebullition, as long as the flame was applied, and when, after the globule became cold, it was heated again, the ebullition was equally renewed; the globule was a glass of a beautiful blueish milky white.

"The beryl is melted with difficulty before the blow-pipe alone, but easily when borax is added."

Emerald of Peru. (Ibid 511.)

The same, only the globule was green, and perfectly transparent.

Olivin—fused into a dark brown globule, almost black.

"It can scarcely be melted by the blow pipe without addition." (Ibid 534.)

Vesuvian—instantly melted into a beautiful green glass.

"It melts before the blow pipe into a yellowish glass." (Ibid 534.)

Leucite—instantly fused into a perfectly transparent white glass; the fusion was attended with strong ebullition, and many ignited globules darted from it and burnt in the air, or rolled out upon the charcoal and then burned. Were they not potassium? This stone contains full 20 per cent of potash: this hint will be resumed below.

"It is not fused before the blow pipe." (Murray III. 534.)

Chrysoberyl—(Cymophane of Hauy) was immediately fused into a greyish white globule. "It is not melted by the blow pipe," (Ibid 499.)

A Chrystalized mineral.

From Haddam, Connecticut, according to the Abbe Hauy it is Chrysoberyl, according to Col. Gibbs, Corundum: it fused with ebullition, and scintillations, and produced a very dark globule almost black.

TOPAZ—of SAXONY, melted with ſtrong ebullition, and became a white enamel. "It is infuſible before the blow pipe, but melts when borax is added." (Ibid 498.)

SAPPAR or Kyanite—perfectly and inſtantly fuſed, with ebullition, into a white enamel.

"It remains perfectly unaltered before the flame of the blow pipe even when excited by oxygen gas." (Ibid 499.)

CORUNDUM—of the Eaſt Indies, was immediately and perfectly fuſed, into a grey globule.

CORUNDUM—of China, the ſame with active ebullition. Corundum "is not fuſed by the flame of the blow-pipe on charcoal even when ſoda or borax is added to it." (Ibid 495.)

ZIRCON—of Ceylon melted, with ebullition, into a white enamel. "It is not melted alone before the flame of the blow pipe, but if borax is added it forms a transparent glaſs." (Murray III. 539.)

HYACINTH—of Expailly fuſed into a white enamel.

"It loſes its colour before the flame of the blow-pipe, but it is not fuſed; it melts with borax into a transparent glaſs." (Ibid 540.)

CINNAMON STONE—inſtantly fuſed into a black globule with violent ebullition.

SPINELLE RUBY—fuſed immediately into an elliptical red globule. "It does not melt before the blow pipe but is fuſed by the aid of borax. [Ibid 497.]

STEATITE—melted with ſtrong ebullition into agreyiſh ſlag.—"It does not melt before the blow pipe, but becomes white and very hard." [Ibid 482.]

Porcelain, common pottery, fragments of Heſſian crucibles, Wedgwood's ware, various natural clays, as pipe and porcelain clay, fire and common brick, and compound rocks, &c. were fuſed with equal eaſe.

During the action of the compound flame upon the alkaline earths, provided they were ſupported by charcoal, diſtinct globules often rolled and darted out from the ignited maſs, and burnt, ſometimes vividly, and with peculiarly coloured flame. From the nature of the

experiments, it will not be eafy to prove, that thefe globules were the bafis of the earths, and yet there is the ftrongeft reafon to believe it; circumftances could fcarcely be devifed, more favourable to the fimultaneous fufion and decompofition of thefe bodies; charcoal highly ignited for a fupport and an atmofphere of hydrogen alfo in vivid and intenfe ignition; that the oxygen fhould be, under thefe circumftances, detached, is not furprifing, but the high degree of heat, and the prefence of oxigen neceffarily burn up the metalloids almoft as foon as produced. If means could be devifed to obviate this difficulty, the blow pipe of Mr. Hare might become an important inftrument of analitical refearch.

We can fcarcely fail to attribute fome of the appearances, during the fufion of the Leucite, to the decompofition of the potafh it contains.

This impreffion was much ftrenghtened by expofing potafh and Soda to the compound flame, with a fupport of charcoal; they were evidently decompofed: numerous diftinct globules rolled out from them, and burnt with the peculiar vivid, white light, and flafh, which these melalloids exhibit, when produced and ignited in the galvanic circuit. It is hoped that thefe hints may induce a farther investigation of this fubject.

The experiments which have now been related in connection with the original ones of Mr. Hare, fufficiently fhew that fcience is not a little indebted to that gentleman for his ingenious and beautiful invention.—It was certainly a happy thought, and the refult of very philofophical views of combuftion, to fuppofe that a highly combuftible *gafeous* body, by intimate mixture with oxygen gas, muft, when kindled, produce intenfe heat: and it is, no doubt, to this capability of perfectly intimate mixture, between these two bodies, that the effects of the compound blow pipe, are, in a great meafure, to be afcribed.

This communication has already been extended farther than was contemplated, but on concluding it, it may be allowable to remark, that there is now, in all

probability no body, except ſome of the combuſtible ones, which is exempt from the law of fuſion by heat. If the primitive earths, and ſuch minerals, as several of thoſe which have been mentioned, above, are fuſible, no doubt can be entertained that all other mixtures and combinations of earths are fuſible alſo: for, ſuch mixtures and combinations are known to be more fuſible than the primitive earths; the metals are more fuſible than the earths, and the diamond along with carbon in its other pureſt forms, appears to be really the only exception; and it is probable that this is only a *ſeeming one*, for, it is scarcely poſſible to expoſe theſe bodies to the heat of the compound blow pipe, without at the ſame time burning them up: could the heat be applied without expoſing them to the contact of oxigen is it not probable that they alſo would be added to the liſt of fuſible bodies?

Yale College, May 7, 1812.

No. XXI.

OBSERVATIONS

On the Comet of 1811.

By JEREMIAH DAY, Prof. of Math. and Nat. Philo. in Yale College.

ON the 9th of September laſt, I began a courſe of obſervations, on the Comet which has lately withdrawn from our view. They were continued, with conſiderable intervals of interruption, till the 13th of January. Though I was poſſeſſed of no inſtrument, which would give the place of the Comet, with the utmoſt preciſion; yet the obſervations were ſufficiently correct, for a determination of the elements of its orbit, with a near approach to exactneſs.

Few Comets have preſented themſelves to our view, under circumſtances more favourable, for obſerving their motions. Many have appeared for a few days, and then, ſuddenly retired. But this was viſible, for ſeverable months, even to the naked eye. During this period, it traverſed a ſpace, of more than 130 degrees, in the heavens. A general idea of the direction of its motion may be obtained, by conceiving a great circle to be drawn through the ſtar η, in the extremity of the tail of the Great Bear; and the bright ſtar in the Eagle. It was near the firſt of theſe, on the 2d of October; and very near the latter, on the 1ſt of December, as Mr. Bowditch had predicted, two months before. It paſſed acroſs the conſtellation of the Great Bear, the head of Aſterion, the right arm of Bootes, the northern part of Hercules, the Eagle, and the bow of Antinous; and when I ſaw it laſt, on the 13th of January, it was a little ſouth of the head of Equuleus. Clouds intercepted the view of it, one or two of the following evenings; then ſucceeded a period of moonlight; and, when the moon had paſſed the full, the

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comet had advanced too near the ſun, to be longer viſible. I had found its place, from time to time, by taking its diſtance from ſome of the principal fixed ſtars, with a ſextant, graduated to every ten ſeconds. The time was given, by a well regulated clock. To diminiſh the errours of obſervation, each of the diſtances was generally meaſured ten or twelve times in ſucceſſion, and a mean taken from the whole. The requiſite corrections, for the refraction of the atmoſphere, were afterwards applied. The following are ſome of the obſerved diſtances. Two or three of the firſt are not probably very correct, as they were taken, when the comet was ſo near the horizon, as to render the view of it obſcure, and the refraction uncertain.

	Apparent	*time.*		*Dist. from*	*Arct.*		*Fr. γ*	*Ursæ.*	*Maj.*
		h.	*m.*	°	′	″	°	′	″
Sept.	9th,	8	9	45	38	26	15	18	10
	13	8	31	42	46	30	11	59	40
	16	8	7	40	26	8	10	0	34
	17	8	17	39	44	26	9	26	38
	18	8	28	38	57	10	8	50	28
				From α	*Lyræ.*		*β Ursæ*	*Min.*	
Oct.	11	8	47	37	12	54	27	33	54
	17	8	52	27	20	0	32	22	39
	18	9	3	25	47	12	33	24	24
							α	*Aquilæ.*	
	27	8	4	13	56	30	41	6	12
	28	8	10	13	6	54	39	27	38
Nov.	3	7	46	11	53	4	30	19	4
	4	7	50	12	17	18	28	53	12
	12	7	18	18	14	7	18	38	4
	14	7	49	20	1	43	16	18	49
	15	7	23	20	54	54	15	14	5
	22	6	52	26	43	17	8	16	34
	24	7	37	28	17	14	6	30	4
Dec.	8	6	8	37	28	24	3	57	48
	10	7	8	38	34	20	5	13	20
	12	7	36	39	89	32	6	25	40
	16	6	53	41	39	3	8	41	25
	20	6	40	43	30	43	10	48	5

This comet came to us, from the regions of the south. It crossed the ecliptic on the 11th of July, and advanced so far to the north, that on the 28th of September, it was within the circle of perpetual apparition of this latitude, and, of course, continued above the horizon, the whole 24 hours. In two or three days from this, it reached its greatest northern declination, about 50 degrees; and then began to return towards the equator. But it did not attain its highest geocentric latitude, till the middle of October. Its apparent place was then, within 28 degrees of the pole of the ecliptic. Its motion, as seen from the earth, was, during the month of September and October, nearly in the arc of a great circle. But the latter part of the time, in which it was visible, it sensibly deviated to the east of its former direction; though, on account of its moderate angular velocity, its elongation from the sun was then rapidly diminishing. The rate of its apparent motion has been less, than that of many other comets. One seen in 1472 moved 40 degrees, and another in 1770, 45° in 24 hours. But the progress of the late comet never exceeded two degrees in 24 hours: and during the latter part of the time, it did not amount to half a degree.

This is a general view, of the *apparent* motion of the comet, to a spectator on the earth. But its *real* motion will be found to be widely different: so much so, that, while the apparent motion was, most of the time, towards the east; its real motion in longitude was westward, or contrary to the order of the signs. To determine the direction and rate of this motion, it was necessary to refer it to the *sun*, as a centre, and to calculate the *elements* of the comet's orbit. These are the Perihelion *distance*, the *time* of passing the perihelion, the *longitude* of the perihelion, the longitude of the *node*, and the *inclination* of the orbit to the ecliptic. To obtain a first approximation, to the time and distance of the perihelion, some of the early observations were made use of. But for the final corrections, it was ne-

ceſſary to take ſuch, as had a greater interval of time between them. The obſervations of the 17th of September, the 17th of October, and the 14th of December were accordingly ſelected: and the elements were corrected by the method of La Place, as given, in Sir Henry Engleſield's treatiſe on the "Determination of the orbits of Comets." To ſucceed with this method, it was neceſſary that the elements ſhould be very carefully prepared. For the convenience of calculation, the three obſervations were reduced to 8 o'clock, mean time at Yale College, by applying to the obſerved diſtances, a proportional part of the diurnal variation. The right aſcenſion and declination of the ſtars, from which the diſtances of the comet had been taken, were found, by comparing the catalogues of Dr. Bradley, De La Caille, Piazzi, Zach, Wollaſton and Pond, in Vince's Aſtronomy, Hutton's Dictionary, Rees' Cyclopedia, and Mendoza's Tables. Theſe generally agreed, within a very few ſeconds, with reſpect to the ſtars in queſtion. The corrections, for preciſion, aberration, and nutation were applied; and from the right aſcenſions and declinations, the latitude and longitude of the ſtars, were calculated to ſeconds; as were alſo the geocentric latitudes and longitudes of the comet. The latter were farther corrected, by applying the equations, for the aberrations of light. The three latitudes and longitudes thus obtained, were,

	Geocen. Lon.	*Latitude.*
September 17,	153° 64′ 36″	38° 39′ 56″
October 17,	221 54 28	62 27 43
November 14,	286 9 15	41 50 53

Taking theſe, and the approximate perihelion diſtance and time, as the baſis of the ſucceeding calculation, I proceeded to derive from the comet's obſerved longitudes and latitudes, the arc which it deſcribed, between the firſt and ſecond obſervation; and alſo between the ſecond and third; for the purpoſe of comparing them, with the correſponding arcs, obtained by taking the difference of the three anomalies. If theſe had been

found to agree; no other correction of the elements would have been requisite, to adapt them to the three observations. But this was not to be expected, on the first trial. There was a difference of several degrees. It was necessary therefore to make a second, and a third hypothesis, in one of which, the perihelion *distance* was changed, and, in the other, the *time* of perihelion. From the errours of the three, the equations were formed, for determining the corrections to be applied to the assumed elements. After two sets of hypotheses, a perihelion distance, and time of perihelion were obtained, which gave the arc described by the comet, between the 17th of September and the 14th of November, and measured by an angle at the sun, within less than half a minute of that deduced from the observed longitudes and latitudes:

The one being	58°	26′	14″
The other	58	25	52
Difference	0°	0′	22″

As this difference is within the limits of the unavoidable errours of observation, I did not attempt to carry the approximation any farther. Some slight variations would probably have been made, by processes depending on a comparison of *all* the observations. But as they would be of little amount; and as *perfect* accuracy would not be attainable after all; I proceeded to calculate the remaining elements, from the two already found. The whole together were as follows:

Perihelion distance 1.0329; the mean distance of the sun from the earth being 1.

Time of perihelion Sept. 12th, 1 o'clock, P. M. mean time at Greenwich.

	s.		
Longitude of the ascending node,	4	20°	22′
Inclination of the orbit to the ecliptic,		73°	4′
Longitude of the perihelion, counted on the orbit,	2	15°	14′
Distance of the perihelion from the node,	2	5°	8′

Motion retrograde.

The following diſtances of the comet from the earth, and the ſun were alſo calculated ;

Diſtances from the earth.

	In semidiam. of the earth's orbit.	*In miles.*
July 10th,	2.411	229,045,000
September 17th,	1.512	143,670,000
October 17th,	1.224	116,290,000
November 14th,	1.591	151,145,000
December 16th,	2.359	224,070,000

Diſtances from the Sun.

	Semidiam. of earth's orbit.	*Miles.*
Sept. 12th	1.0329	98,125,500
Sept. 7th and 17th,	1.0367	98,490,000
Sept. 2d and 22d,	1.0470	99,470,000
Aug. 23 and Oct 1,	1.081	103,000,000
Aug. 8 and Oct. 17,	1.189	113,000,000
July 23 and Nov. 1,	1.323	126,000,000
July 11 and Nov. 14,	1.463	139,000,000
June 23 and Dec. 1,	1.655	157,000,000
June 9 and Dec. 16,	1.831	174,000,000
May 23, 1811, and Jan'y 1, 1812,	2.022	192,000,000
April 22, 1811, and Feb. 1, 1812,	2.385	227,000,000

In addition to the elements given above, one thing farther is neceſſary to complete the theory of the comet's motion—the period of its revolution. This is an article on which much labour has heretofore been beſtowed, with very little ſucceſs. Two methods are obviouſly ſuggeſted, for determining the time of a comet's return. The moſt direct of theſe, is to derive, by calculation, the figure and dimenſions of the *whole* orbit, from that ſmall part of it, which is deſcribed, while the comet is viſible. It reſults from the well known laws of gravitation, that any body, moving round the ſun, and influenced by the attraction of no other body, muſt move in one of the three conic ſections, the ellipſis, the parabola,

or the hyperbola. If it is ever to return, in a regular orbit, it muſt revolve in an ellipſis. If its motion is in either of the two other figures, after having once paſſed the perihelion, it will continually recede from the ſun, and will return no more; unleſs its direction ſhould be changed, by the attraction of ſome other body. In each of theſe caſes however, if a portion of the path of a comet could be obtained by obſervation, with *perfect exactneſs*; from this might be deduced, the remaining parts of the orbit, on the ſuppoſition, that its figure ſhould remain unaltered. But here two inſuperable difficulties occur, one from the errours of obſervation, the other from the liability of the comet, to be diverted from its courſe, by the attraction of the planets, and perhaps of the fixed ſtars. The ground of theſe difficulties, lies not in the proceſses for *calculating* the orbit; but in taking the obſervations, and in the change of the orbit itſelf. Errours too minute to be avoided, even by the niceſt inſtruments hitherto in uſe, might be ſufficient, in certain caſes, eſpecially where the orbit is very eccentric, to make a variation of many years, in the periodical time. The moſt able computers, have accordingly differed whole centuries, in the periods, which they have reſpectively aſſigned to the ſame comet. The only caſes to which calculation can be applied, with any hope of ſucceſs, are thoſe in which the time of revolution is very ſhort. The comet of 1770, has been ſuppoſed to be one of this claſs. Lexell, Pingre, and Burckhart all agree in giving it a period of about five years and an half. There is reaſon to believe, that this is the orbit, which really correſponds with that part of its motion, which was obſerved. But notwithſtanding this, the comet has never been ſeen ſince; though it ought to have returned ſix or eight times, in the intervening forty years.

If its orbit was truly aſſigned, it ſeems it muſt have been ſince altered, by the attraction of the planets or of ſome other body, whoſe influence may be ſufficient, not only to vary materially the time of revolution; but

even to change the comet's path, from an ellipſis, to a parabola or hyperbola, ſo that it ſhall *never* return.

The ſecond method, which has been adopted, for aſcertaining the periodical time of a comet; and, on which conſiderable reliance has been placed, is, to compare the elements which are commonly computed, with thoſe of all the other comets, on which calculations have been made. If ſeveral of them are found to have the ſame elements; it is concluded they muſt be one and the ſame comet. For it is ſcarcely credible that in the endleſs variety which is found to prevail, in the directions and rate of their motions, there ſhould be any two, which ſhould have preciſely the ſame perihelion diſtance, the ſame longitude of their nodes, the ſame place of perihelion, and the ſame angle of inclination to the ecliptic: and, in addition to all this, that they ſhould appear at intervals of time ſo regular, as to correſpond with the ſucceſſive returns of the ſame comet. On this ground, it has been ſuppoſed, that the comet of 1759 has a period of about 75 years, becauſe one with ſimilar elements was ſeen in the years 1456, 1531, 1607, and 1682. But no ſuch correſpondence is found, in the caſe of the late comet. From a compariſon of its elements with others whoſe motions have been heretofore obſerved and calculated; it will be ſeen that this is one, which is not included in the liſt. In perihelion diſtance, it nearly agrees with one, which appeared in 1718; but differs from it, more than 40 degrees, in the inclination of the orbit. In the longitude of the node, it is within leſs than a degree of that in 1759; but varies materially from it, in the longitude and diſtance of the perihelion. In the inclination of the orbit, it differs but little from one in 1097 and another in 1763; but has no agreement with them, in the other elements. So that this method of determining the periodical time fails, in its application to the preſent caſe. And even, if the elements of this comet, had been found to agree, with thoſe of any preceding one; it would have ſerved rather to ſhew, in what interval of time it *has* returned, than to give us

aſſurance, that it *will* return, at the ſame interval, hereafter. The reaſons of this, have already been ſtated to the Academy, by Col. Mansfield, in his ingenious remarks, on the comet of 1807.

The motion of the late comet, as ſeen from the ſun, was nearly north and ſouth; inclining however, about 17 degrees, towards the northweſt and ſoutheaſt. It paſſed northward, through the conſtellations of the Lion, the Great Bear, and the Camelopard. On the 19th of September, to a ſpectator in the ſun, it was within three degrees of that part of the heavens, to which the axis of the earth is directed. It reached its greateſt northern heliocentric latitude 73 degrees, on th 1ſt of October; and then returned, through Cepheus, the left wing of the Swan, Equuleus and Aquarius. It will proceed through the Microſcope, the Indian, and the Octant; till, at its greateſt diſtance from the earth, it will be between the Chameleon and the Flying-fiſh, within 17 degrees of the ſouth pole of the ecliptic. Should it viſit us again, it will return through the Ship, the Air-pump, Hydra and the Sextant.

This comet has not approached ſo near to the ſun, as moſt of thoſe, whoſe elements have been calculated. Its leaſt diſtance, is 98 millions of miles; a little greater than that of the earth from the ſun. Its path lies between the orbits of the earth and Mars. It is the opinion of Newton, that no comet is ever ſeen by us, when farther diſtant than Jupiter. Of about one hundred, whoſe elements have been calculated, all, except four, have come nearer to the ſun than Mars. Theſe four fell between Mars and Jupiter. About 20 came between Mars and the earth; 15 between the earth and Venus; nearly 30 between Venus and Mercury; and about the ſame number, within the orbit of Mercury. The remarkable one of 1680, came much nearer to the ſun, than any other: ſo near, as to be heated, according to Newton, 2000 times hotter than red hot iron. Its leaſt diſtance, from the ſurface of the ſun, was not equal to a fifth part of his diameter.

There is no danger, that the late comet will ever ftrike the earth; unlefs its orbit fhould, hereafter, be materially changed. Its leaft diftance, in the prefent inftance was more than an hundred million miles. This was about the middle of October. Its neareft approach to the *orbit* of the earth, was on the 11th of July, about 40 million miles. It can never come much nearer than this, without a change in the pofition or figure of its orbit. Its rate of motion, when neareft the fun, was 95,000 miles in an hour. This is a velocity, 120 times greater than that of found, or a cannon ball.

For the purpofe of determining, if practicable, the *size* of the comet, I viewed it feveral times, through a three feet reflecting telefcope, with a magnifying power of 140. But I was unable to perceive any nucleus, with a difk fufficiently defined, to admit of meafuring its diameter. This will not appear furprifing, when it is confidered, that out of 15 or 20 comets, which Dr. Herfchel has had an opportunity of obferving, there were only two or three, on which he was able to difcover any regular difk, even with the very powerful telefcopes in his poffeffion. For want of a proper regard to the diftinction between the different parts of a comet; no great dependence is to be placed, upon the accounts given us, of the fize of thofe, which were formerly feen. They have been frequently reprefented, as larger than any of the planets; and, in fome inftances, as appearing nearly equal to the fun and moon. Thefe ftatements may be true, if they are meant to refer to the *whole* of the luminous fpot, or body of light, which is perceived, by the naked eye. But this, in many cafes at leaft, confifts of three parts —the nucleus, the head, and the coma. The nucleus appears to be a compact fpherical body, with a circumference regularly defined, like a planet. The head, is a body of denfe light, which, like an atmofphere, furrounds the nucleus. The coma, is a fpace occupied by a fainter light, extending confiderably farther round, than the head. All thefe may be fo blended, to the view of the naked eye, as to appear to conftitute but

one body of light; fo that the diameter of the coma may be taken, for the diameter of the comet itfelf. A telefcope, by expanding the head, often makes it appear lefs bright, and lefs diftinct. It fpreads it out, into a kind of nebula, which has no well defined boundary; but which may, notwithftanding, be miftaken for the nucleus. The latter is generally too fmall, to be diftinctly perceived, by ordinary telefcopes; and in many inftances, cannot be difcovered, by thofe of the greateft magnifying power. That of 1807, according to Dr. Herschel, fubtended an angle of only one or two feconds; while the diameter of the coma was two or three hundred times as great La Place and others have even advanced the opinion, that the whole body of the comet is fometimes a mere collection of aeriform fluids, moft condenfed near the centre, but containing no fubftance fufficiently compact, to obftruct entirely the paffage of light. In the prefent inftance, I obferved nothing, which could either invalidate or confirm, fuch an hypothefis. The head appeared, like an obfcure cloud or mift. The coma was nearly of the fame brightnefs with the tail. A darker fpace, or a zone of weak light, very perceptibly fainter than either the head, or the coma, intervened between the two.

The tail of this comet, was more fplendid, than is common: though fome others have exceeded it, both in brightnefs and extent. That of 1680, was two or three degrees in breadth, and about 70, in length. That of 1759 was, according to Pingre, 90° long. One in 1618 is faid to have extended more than a hundred degrees.

The length cannot be meafured, with exactnefs. For the extremity does not terminate at once; but gradually diminifhes in brightnefs, till it is loft, in the ftill fainter light of the fky. The dimenfions will vary, according to the ftate of the atmofphere. They will even appear different, to different eyes, at the fame time. About the middle of October, I judged the tail of this comet to be nearly or quite 15 degrees long. It could not be lefs than twelve. If it be taken at 12° on the 17th;

and the breadth at half a degree ; it will be found, by calculation, that the length in miles, was 40,000,000 ; and the breadth 1,000,000 : the whole occupying a ſpace, which would not be filled by 60 million ſuch globes as our earth.

Stars were frequently to be ſeen through the tail ; though they were conſiderably obſcured. That ſide of it, which was turned towards the part of the heavens, to which the comet was moving, was a little convex, as uſual, and brighter than the oppoſite ſide. I repeatedly obſerved a dark line, like a ſhadow, extending from one end of its tail, to the other ; and paſſing nearly through the middle, but a little further from the advancing ſide, than from the other. The whole appearance was ſuch, as to correſpond very well with the ſuppoſition, that the tail was hollow. The light was denſeſt on each ſide, and gradually diminiſhed towards the middle, where was a narrow ſpace, almoſt as dark as the neighbouring parts of the ſky. This peculiarity has not been mentioned, in the accounts of other comets, except in a few inſtances. Hevelius ſtates, that one which he ſaw in 1665, caſt a ſhadow through the middle of the tail. A ſimilar appearance was obſerved in the comet of 1744 ; and alſo by Caſſini, in that of 1680.

I have not entered into any ſpeculations on the *nature* and *uſe*, of this wonderful train of light, which is as unaccountable, to the aſtronomer, as to the vulgar obſerver. Some extravagance of conception is certainly excuſable, in attempting to explain the conſtitution of a luminous object, which occupies a greater ſpace, than all the other bodies in the ſolar ſyſtem. But the ſchemes which have hitherto been propoſed, for this purpoſe, are rather to be conſidered as diſplays of the power of imagination ; than ſpecimens of the exerciſe of ſound and ſober reaſon. Thoſe who have a taſte for theſe viſionary hypotheſes, may eaſily contrive them for themſelves ; or may find, in the common aſtronomical works, a very convenient aſſortment of them, adapted to the fancy, of almoſt every deſcription of readers.

Yale-College, *March* 20*th*, 1812.

No. XXII.

CALCULATION

Of the Longitude of Yale-College, from the Solar Eclipse of September 17th, 1811.

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IT is important, that the Latitude and Longitude of places, especially of those in which astronomical observations are to be made, should be settled, with as much accuracy, as the nature of the case will admit.

The Latitude is easily obtained, from the meridian altitudes of the sun, and the fixed stars. But some more complicated process is necessary, for determining the Longitude. Most of the methods in use, for this purpose, depend on the motion of the moon in her orbit. Several of them, however, cannot be relied on, to give the longitude, with any great degree of accuracy. That founded on the observation of solar eclipses, has an advantage in this respect, over most of the others. As we had seen no circumstantial account of any attempt, to ascertain the longitude of Yale-College; we made preparations, to avail ourselves of the opportunity furnished, by the eclipse of the sun, of Sept. 17th 1811. The going of the clock, from which the time was to be taken, was carefully attended to, for several months preceding. Its rate was found, by observing the passage of the sun and fixed stars, across the meridian. A transit instrument, with a telescope of about twenty inches focus, had been fixed, under the cupola of the Lyceum, nearly two years before. Care had been taken, during this period, by repeated observations of the pole star, to obtain an accurate adjustment of the telescope in the plane of the meridian.

When the meridian was ſatisfactorily determined, an object was placed in view, nearly two miles diſtant, by which the inſtrument might afterwards be eaſily adjuſted at pleaſure.

The day of the eclipſe was remarkably fine. Scarce a cloud was to be ſeen, the whole time the ſun was above the horizon. There was little or no wind, to diſturb our obſervations. One of us was ſtationed at the clock; while the other was looking at the ſun, through a three feet reflecting teleſcope. The eclipſe was perceived, almoſt, if not quite, at the inſtant of its commencement. The time of the end was obſerved, with no leſs exactneſs.

The rate of the clock was determined, by obſervations on the meridian tranſits, of the ſun, and of the fixed ſtars, on that and the preceding and following days. It was found to loſe eight ſeconds, in twenty-four hours. The paſſage of the ſun, acroſs the meridian, on the day of the eclipſe, was very carefully obſerved with the tranſit inſtrument. The time, by the clock, was 11h —*54′*—*9.5″*

The following were the obſervations of the beginning and end of the eclipſe.

	h.	m.	s.
Beginning, by the clock, - - - -	0	38	22
Clock slower than the sun, - - -	0	5	50.5
Allowance for the rate of the clock, - -	0	0	0.2
Apparent time of beginning, - - -	0	44	12.7
Time of end, by the clock, - - -	3	46	5
Clock slower than the sun, - - -		5	50.5
Allowance for the rate of the clock, - -	0	0	1·3
Apparent time of end, - - - - -	3	51	56.8

To find the latitude of the place, the meridian altitude of the ſun, was repeatedly taken, with an Equatorial Inſtrument, containing a teleſcope of 18 inches focus. The mean of twelve obſervations, was very nearly 41°—18′. The latitude is taken at this, in the following calculations; and the reductions of parallax

and latitude are made, according to that estimate of the figure of the earth, which gives the proportion of the polar to the equatorial diameter, as 300 to 301.

From these data, the longitude might be calculated, if the solar and lunar tables could be relied on, as perfectly correct. But it is well known that they are liable to an errour, which might materially affect the result. It is necessary, therefore, that the tables should be corrected, or that the time of true conjunction should be obtained, from observations, at some place or places, whose longitude is already known. For this, we are indebted to a very obliging communication from Nathaniel Bowditch Esq. of Salem; on whose accuracy in calculation, full reliance may be placed: and who had undertaken to collect the observations, which were made on the eclipse, in different parts of the United States. He finds the time of true conjunction to be at 6h. 57′. 05.8″ apparent time at Greenwich; the longitude of the sun and moon, at that time, 173° 56′ 32.4″; and the moon's Latitude 36′ 40.2″ North. The Following are the calculations for the longitude at Yale College.

For the parallaxes, &c. at the beginning of the Eclipse.

	h.	m.	s.
Apparent time of beginning at Yale-College,	0	44	12.7
Assumed difference of Longitude, -	4	51	50
Apparent time at Greenwich, - -	5	36	02.7
Sun's Right Ascension in time, -	11	37	34
Sun past meridian, - - - - -	0	44	12.7
Correction for the sun's advance, - -			+0.7
R. ascension of mid-heaven, in time, -	12	21	47.4
Do. - - - in degrees, -	185°	26′	51″
Distance of meridian from Capricorn, -	84	33	09
	h.	m.	s.
Apparent time of beginning - - -	5	36	02.7
Do. of conjunction, (from Mr. Bowditch) -	6	57	05.8
Time from beginning to conjunction, -	1	21	03.1
Moon's Lon. at conjunction, - -	173°	56′	32″.4

Motion in 1 hour, (by Burg's tables,) -			0° 29′ 32″.7
Do. in 21m. 3.1s. - - - - -			10 22.9
Moon's Lon. at the beginning - -			173 16 36.8
Moon's Lat. at conjunction, - - -			36 40.2 N
Motion in 1 hour - - - - - -			2 43.22
Do. in 21m. 3.1s. - - - - -			57.28
Lat. at the beginning, - - - -			32 59.7
Dist. merid. from Cap.	84° 33′ 09″	Cos.	8.9774201
Co. Lat. reduced, -	48 53 20	Tan.	10.0591364
Arc I. - - -	6 12 30	Tan.	9.0365563
Dist. of the poles, -	23 27 42		
Arc II. - - -	17 15 12	Sin.A.C.	0.5278330
Arc I. - - -	6 12 30	Sin.	9.0340022
Dist. of merid. from Cap.	84 33 09	Tan.	11.0206140
Dist. of the nonagesimal from Cancer,	75 20 34	Tan.	10.5824492
Add - -	90		
Lon. of nonagesimal,	165 20 34		
Lon. of the moon	173 16 36.8		
Dist. of the moon E. fr. non.	7 56 02.8		
Dist. of the non'l. fr. Cancer	75 20 34	Cos.	9.4031818
Arc II.	17 15 12	Tan.+10	19.4921624
Alt. of nonagesimal	50 49 44	Tan.	10.0889806
Moon's equa. hor. paral. 53′ 59″.9 =	3239″.9		
Reduc. for Lat. 41° 18′	—4.7		
Moon's reduced hor. par.	3235.2		
Sun's horizontal parallax -	8.7		
Hor. par. of moon from sun,	3226.5	Log.	3.5087317
Altitude of the nonages.	50 49 44	Sin.	9.8894491
Moon's Lat. - -	0 32 59.7	Cos.A.C.	0.0000200
		Log.	3.3982008
Dist. of the moon from non.	7 56 02.8	Sin.	9.1899868

Approximate par. in Lon.	5′ 45.3″	Log.	2.5381876
			3.3982008
Dist. of moon from non. + par. in lon.	8 1 48.1	Sin.	9.1451717
Correct par. in lon. 349.5″	= 5 49.5	Log.	2.5433725
Hor. par. of moon from sun,	3226.5″	Log.	3.5087317
Alt. of the non. -	50 49 44	Cos.	9.8004686
1st Part of par. in lat. 2038″	= 33 58	Log.	3.3092003
2d Part - - - -	0.7		
Correct par. in Lat. -	33 58.7		
Moon's true Lat. -	32 59.7	N.	
Moon's apparent lat. -	0 59	S.	
Moon's hor. semid.	14′ 44.27″		
Inflexion, -	—2		
	14 42.27		
Augment. for 50° alt.	+ 10.73		
Moon's cor. semid.	14 53 = 893″		
Sun's semid.	15 57.2		
Irradiation,	— 3.5		
Sun's cor. semid.	15 53.7 = 953.7		
Sun and moon's semidiameters	1846.7		
Moon's appar. Lat. - -	59		
Sum, - - - -	1905.7	Log.	3.2800545
Difference, - - -	1787.7	Log.	3.2522946
			2)6.5323491
Appar. Lon. of moon from sun,	1845.76	Log.	3.2661745
Par. in lon. - - -	+ 349.5		
True lon. of moon from sun,	2195.26		
Moon's hourly mo. in lon.	29′ 32.7″		
Sun's do. - - -	2 26.55		

Hourly mo. fr. sun 27′ 06.15″=1626.15″		Log. A.C.	6.7888394
One hour, - - -	3600	Log.	3.5563025
Lon. of moon from sun, -	2195.26	Log.	3.3414860

Time from beginn. to conjunction,	h. m. s. 1 20 59.9=4859.9	Log.	3.6866279
Time of begining,	0 44 12.7		
Time of conj. at Y.C.	2 05 12.6		

For the Parallaxes, &c. at the end.

	h. m. s.
Apparent time of end,	3 51 56.8
Assumed differ. of lon.	4 51 50
Apparent time at Greenwich,	8 43 46.8
Sun's Right-ascen. in time,	11 38 02
Sun past meridian,	3 51 56.8
*Cor. for the sun's advance,	+ 3.4
R. Ascen. of mid heaven	15 30 02.2
Do. in degrees,	232° 30′ 33″
Dist. of merid. from cap.	37 29 27
	h. m. s.
Appa. time of end,	8 43 46.8

* NOTE. This correction, which depends on the difference in the equation of time, on two successive days, is generally omitted, in the rules which are given, for finding the right ascension of midheaven; probably because it is of small account. But, in strict propriety, it ought to be included. In the present instance, it will make a difference, of more than a minute, in the longitude of the nonagesimal. The equation of time is 21 seconds greater, on the 18th of Sept, than on the 17th. The sun therefore, in twenty four hours, not only makes a complete revolution; but, in addition to this, falls to the west of the meridian, such a distance, as corresponds to 21 seconds of time. A proportional part of this, should be allowed, for the time between 12 o'clock, and the end of the eclipse.

Do. of conjunction,	6 57 05.8		
Time from conjunc. to end	1 46 41		
Moon's lon. at conjunc.	173° 56′ 32.4″		
Motion in one hour,	29 33.2		
Do. in 46m. 41s.	22 59.6		
Moon's long. at the end,	174 49 05.2		
Moon's lat. at conjunc.	36 40.2 N,		
Motion in one hour.	2 43		
Do. in 46m. 41s.	2 06.8		
Moon's lat. at the end,	41 30		
Dist. merid. from cap.	37° 29′ 27″	Cos.	9.8995200
Co. lat. reduced,	48 53 20	Tan.	10.0591364
Arc. I.	42 16 37	Tan.	9.9586564
Dist. of the poles,	23 27 42		
Arc. II.	18 48 55	Sin. A. C.	0.4914459
Arc. I.	42 16 37	Sin.	9.8278310
Dist. of merid. from cap.	37 29 27	Tan.	9.8848366
Dist. non. from cap.	57 59 39	Tan.	10.2041135
Subtract this from	270		
Lon. nonages.	212 0 21		
Lon. of the moon,	174 49 05		
Moon from non.	37 11 16		
Non. from cap.	57 59 39	Cos.	9.7242805
Arc. II.	18 48 55	Tan.+10.19,	5324044
Alt. of non.	32 44 09	Tan.	9.8081239
Hor. par. of moon fr. sun.	3227.2″	Log.	3.5088124
Alt. of nonages.	32 44 09	Sin.	9.7330098
Moon's lat.	0 41 30	Cos. A. C.	0.0000316
		Log.	3.2418538
Moon from non.	37 11 16	Sin.	9.7813454
Approx. par. in long.	17 35	Log.	3.0231992
		Log.	3.2418538
Mo. from non. + par.	37 28 51	Sin.	9.7842577

Cor. par. in long.	1062″ = 17′ 42″	Log.	3.0261115
Hor. par. mo. from sun	3227.2	Log.	3.5088124
Alt. of the non.	32 44 09	Cos.	9.9248852
1st part of par. in lat.	45 14.54	Log.	3.4336976
2d part of do.	.16		
Cor. par. in lat.	45 14.7		
Moon's true lat.	41 30		
Moon's appar. lat.	224.7 = 3 44.7 S.		
M's. semid.-inflex.	14 42.5		
Aug. for 25° 30′ alt.	+6.1		
Mo's. cor. semid.	14 48.6		
Sun's do.	15 53.7		
Sun and mo's. sem.	30 42.3 = 1842.3″		
Mo's. app. lat.	224.7		
Sum	2067.	Log.	3.3153405
Difference,	1617.6	Log.	3.2088711
			2)6.5242116
Appar. long. of moon from sun	1828.55	Log.	3.2621058
Par. in Lon.	1062.		
True long. of moon from sun,	2890.55		
Mo's. hourly mot.	29 33.15		
Sun's do.	2 26.55		
M's. hourly mot. from sun,	27 06.6 = 1626.6	Log.A.C.	6.7887192
One hour,	3600	Log.	3.5563025
Long. of m. fr. sun	2890.55	Log.	3.4609805
	h. m. s. *s.*		
Time fr. conj. to end	1 46 37.4 = 6397.4	Log.	3.8060022
App. time of end	3 51 56.8		

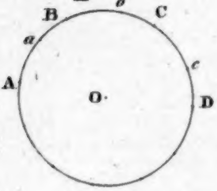
App. time of conj.	2	05	19.4
Do. cal. from begin.	2	05	12.6
Mean,	2	05	16
Conj. at Greenwich	6	57	05.8

	h	m	s	°	′	″
Dif. of long.	4	51	49.8=	72	57	27.

It would have been desirable, to obtain a correction of the tables, from observations, made at the first meridian at Greenwich; that our calculation might not be affected, by the small errours, to which the longitudes of all places in the United States are liable. But as this eclipse was not visible in Europe, we have had recourse to observations made here, to determine the latitude and longitude of the moon, and the true time of conjunction.

Yale College, Nov. 24, 1812.

1



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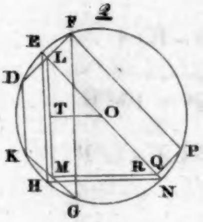

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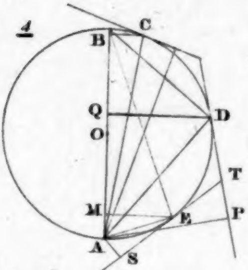

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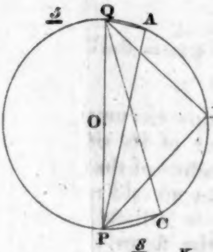

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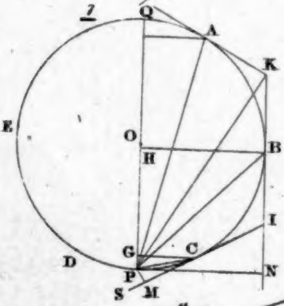

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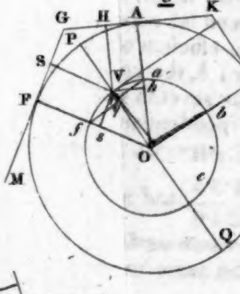

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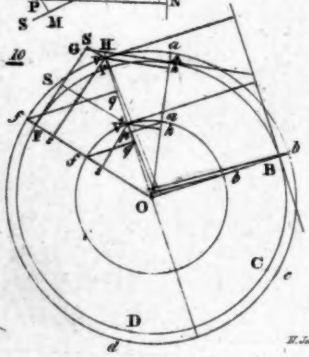

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Pl. III.